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Dear Gus:

I'll be interested to hear the results of the UV experiment on lysis-inhibited cells, however it comes it, and with due account to your reservations. If you're going to Caltech, you'll probably hear no end of lysogenicity discussion.

I doubt very much whether the problems of linkage in K-12 are very pertinent to the situation in phage. I have every reason to suppose that the peculiarities we have found are not inherent in the E. coli genetic system, but are due to chromosomal aberrations in the stocks we have used. Some notion of the basis of this idea will be seen in print in: Cavalli. 1950 La sessualita nei batteri. Boll. I. sieroter. Milan. 29: 1-9; Newcombe and Nyhola 1950 Anomalous segregation in crosses of E. coli, Amer. Nat., 84: 457-465; my abstract in Genetics, Jan. '50. There is a more involved discussion in the GSH ms., but I'm afraid I don't have a copy with all of the tables and figures that I can send you. It boils down to the fact that, by the criteria of my 1947 paper, Bl; Mal; Xyl; and Lac are each linked to M, but not in any linear sequence. The "branched" chromosome idea has been somewhat misunderstood: what I have meant is the likelihood that we have a reciprocal translocation between two chromosomes, with a break-point near M. This would give such a genetiv picture. We are painfully building up new stocks, without the use of artifiéial mutagens, in hopes of developing a less aberrant picture. I should say that Rothfels has a paper in press (Genetics) with a very nice, sound confirmation of the linear order of the segment to the "right" of M with the loci Lac, V_1, T, L. I think that linearity is still the fundamental system in coli, and each aggregate of factors still behaves all right. Would repetitive mating cause much trouble in assessing linear order in phage?

Sincerely,

Joshua Lederberg